

Though it has been remarked, that a calm always attends an earthquake; no such thing happened now, a fine gale of wind blowing before and after, as well as during the time of, the shock.

The sky was serene, interspersed with flying clouds.

The sun, which shone very bright, immediately after the earthquake was surrounded by a very large halo, which lasted about an hour, and gradually disappeared.

XXXI. *An Account of a Treatise in Latin, presented to the Royal Society, intituled, De admirando frigore artificiali, quo mercurius est congelatus, dissertatio, &c.* a J. A. Braunio, Academiæ Scientiarum Membro, &c. by William Watson, M. D. R. S. S.

To the Royal Society.

Gentlemen,

Read May 21, 1761. **V**ERY early last year, we were informed, that at Petersburg, by the means of artificial cold, the mercury in thermometers had been condensed to so great a degree, as to become perfectly fixed and solid: but as this information was received only in a loose way, from the public gazettes, the opinions of philosophers here were suspended, in relation to their giving credit to this

this very extraordinary phænomenon, until the truth of it could be sufficiently authenticated. This has very lately been done by Professor Braun, who first made the experiments, and who presented an account of them to the Royal Academy at Petersburg, a printed copy of which has been communicated by him to the Royal Society.

Professor Braun observes, that every age has its inventions, and that the discovery of some things seem to be reserved for particular persons. To this, the history of sciences in all ages, more particularly of the late and the present, bears witness sufficiently, by the invention of the air-pump, barometers, thermometers, optical instruments, electricity, more particularly the natural, artificial magnets, phosphorus, the discovery of the aberration of light, and of many other things in natural philosophy. He does not know, whether the congelation of mercury, which it was his good fortune to discover, may not be ranged among these: for who did not consider quicksilver, as a body, which would preserve its fluidity in every degree of cold? Neither was the fact otherwise, if this is understood of natural cold, such as it has been found in any part of the globe, hitherto discovered. But if it should happen, that the natural cold should ever be so intense as artificial cold has been found to be, the whole globe would have a different face, as men, animals, and plants, would certainly be destroyed. He did hint some time since, in a dissertation upon the degrees of heat, which certain liquors and certain fluids would bear before they boiled, and the degrees of cold they respectively bore, before they were converted into ice, that there was a suspicion, that the
mercury

mercury in some of the barometers and thermometers made use of for experiments in Siberia had been frozen: but since that in greater degrees of cold, the mercury continued fluid in other barometers and thermometers, the immobility and hardness observed in some of these instruments, was attributed more probably to the lead or the bismuth, with which the mercury had been adulterated, and was not considered as a real freezing of the mercury: but this has been since put out of all doubt; since it is certain, that pure mercury would not freeze under such small degrees of cold, great as they were for natural cold. The experiments, which the professor made, in order to congel mercury, demonstrate this most evidently; besides which, they exhibit new phenomena.

There happened at Petersburg, on the 14th of December 1759, a very great frost, equal if not more intense than any which had been observed there: for, between nine and ten o'clock in the morning, Delisle's thermometer stood at 205; at seven o'clock, at 201; which last was the greatest degree of cold, that had been observed at Petersburg, either by himself or others. At one o'clock at noon, the thermometer stood at 197. Mr. Braun had been employed, several days before this, in observing the several degrees of cold, which different fluids would bear, before they were converted into ice; partly to confirm those things which he had already laid before the academy; and partly to make experiments upon liquors, which had not yet been examined; as on the days between the 7th and 14th the cold was
intense

intense enough to be between the degrees of 181 and 191.

When the natural cold was so intense as to be at 205, Professor Braun conjectured, that it was of all others the most proper occasion to try the effects of artificial cold; not doubting, but that artificial cold would be increased in proportion as the natural was more intense. Aquafortis, which was found by the thermometer to be 204 degrees cold, was the greatest part of it frozen, the ice having the appearance of crystals of nitre; which, however, immediately dissolved in a small degree of heat. This aquafortis, which though frozen at the sides, was liquid in the middle, was poured upon pounded ice, in that proportion which was directed by Fahrenheit, the first person who made artificial cold with spirit of nitre. But before the professor made this experiment, he, by examination, found, that both the ice and aquafortis were of the temperature with the air, which was then 204. Upon the first pouring, the mercury fell 20 degrees; this spirit was poured off, and fresh put on, several times; but it was possible, by these means, to introduce no more than 30 degrees of cold; so that the mercury in the thermometer fell no lower than 234. Since therefore Fahrenheit could not produce cold greater than that of 40 below the cypher of his thermometer, which corresponds with 210 of that employed by Professor Braun; nor Reaumur, nor Muschenbroek, who often repeated the same experiment, our author was upon the point of giving up this pursuit; as considering this as the greatest degree to which artificial cold could be carried; thinking it sufficient honour to himself,

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to have added 20 degrees to the cold formerly known.

But reflecting, that this was not all the fruit he expected from these experiments, he determined to pursue them; but at the same time, however, to vary the manner of them. By good fortune, his ice was all gone, and he was compelled to use snow in its stead, after having first tried, and found the snow of the same degree of cold with the air, at this time 203. The snow, the thermometer, and the aquafortis, being of the same temperature, he immersed the thermometer in snow, contained in a glass; and, at first, only poured a few drops of the aquafortis upon that part of the snow, in which the thermometer was immersed; upon which he observed the mercury to subside to 260. Elated by this remarkable success, he immediately conceived hopes, that these experiments might be carried further: nor was he deceived in his expectations; for repeating the experiment in the same simple manner, he poured on only some more aquafortis, and immediately the mercury fell to 380. Upon which he immersed the thermometer in another glass filled with snow, before it had lost any of this acquired cold; and at length, by this third experiment, the mercury subsided to 470 degrees. When he observed this enormous degree of cold, he could scarce give credit to his eyes, and believed his thermometer broke. But, to his infinite satisfaction, upon taking out his thermometer, he found it whole; though the mercury was immovable, and continued so in the open air twelve minutes. He carried his thermometer into a chamber, where the temperature of the air was 125 degrees;

grees; and, after some minutes, the mercury being restored to its fluidity, began to rise. But to be certain, whether this thermometer had received any injury, and whether it would yet correspond with his thermometer, which he keeps as a standard, he suspended them together, and in twenty minutes the thermometers corresponded one with the other.

The thermometers, which our author usually employs, have a spherical bulb, and their scale is divided into 1200 parts, of which 600 are above the cypher, which denotes the heat of boiling water, and 600 below that heat. A thermometer of this construction was used in investigating the heat of boiling mercury and oils. He had another thermometer, of which the scale went no lower than 360 degrees below the cypher, denoting the heat of boiling water. He repeated the former experiment with this, and the mercury very soon descended so, that the whole was contained in the bulb, which, however, it did not quite fill. The mercury in this bulb was immoveable, even though he shook the thermometer; until about a quarter of an hour, it began to ascend in the open air; and it continued to ascend, till it became higher than the circumambient air seemed to indicate. He was struck with this extraordinary phænomenon, and very attentively looked at the mercury in this thermometer, and found certain air bubbles interspersed with the mercury, which were not in that of the other thermometer. From these, and other experiments (it would be unnecessary to recite them all), he was satisfied, that the mercury in these thermometers had been fixed and congealed by the cold.

Hitherto our professor had only seen the mercury fixed within the bulb of his thermometers. These he was unwilling to break. He was, however, desirous of examining the mercury in its fixed state, and therefore determined to break his thermometers in the next experiments. It was several days before he got other thermometers, which exactly corresponded with those he had already employed.

When these were procured, the natural cold had somewhat relented. In the former experiment, the thermometer stood at 204; it was now at 199. In making the experiment, he varied the manner a little. He first put the bulb of the thermometer into a glass of snow, gently pressed down, before he poured on the aquafortis; he then, in another glass, poured the aquafortis upon the snow, before he immersed his thermometer therein; he then, in like manner, put the snow to the aquafortis, before he put his thermometer therein. Which ever of these ways he proceeded, he found the event exactly the same; as the whole depended upon the aquafortis dissolving the snow. When he had proceeded so far, as to find the mercury immovable, he broke the bulb of the thermometer, which had already been cracked in the experiment, but the parts were not separated. He found the mercury solid, but not wholly so, as the middle part of the sphere was not yet fixed. The external convex surface of the mercury was perfectly smooth; but the internal concave one, after the small portion of mercury, which remained fluid, was poured out, appeared rough and uneven, as though composed of small globules. He gave the mercury several strokes with the pestle of a mortar, which stood
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near him. It had solidity enough to bear extension with these strokes; its hardness was like that of lead, though somewhat softer; and, upon striking, it sounded like lead. When the mercury was extended by these strokes, he cut it easily with a penknife. The mercury then becoming softer by degrees, in about twelve minutes it recovered its former fluidity, the air being then 197. The colour of the congealed mercury did scarce differ from that of the fluid: it looked like the most polished silver, as well in its convex part, as where it was cut.

The next day, the cold had increased to 212 degrees, which was 7 degrees beyond what it had ever before been observed at *Peterburg*. The season so much favouring, he thought it right to continue his pursuit, not only in further confirmation of what he had already observed, but to investigate new phenomena. In two thermometers, he observed the same facts in relation to the congealing of mercury, as he did the preceding day. In the bulbs which he broke, the whole of the mercury was not fixed, as a very small portion, much less than that of the preceding day, continued fluid. He treated this mercury as he did the former; he beat it with a pestle, he cut it, and every thing was thus far the same. But he saw a very great difference in relation to the descending of the mercury in the thermometer, the like of which did not occur to him, neither in the former nor any of the subsequent experiments. From the former ones it appeared, that the mercury in the first experiment had only descended to 470, when it became immoveable, though the glass bulb was not cracked. In the experiment of the 25th, it descended

to 530; and in two thermometers on the 26th, to 650. But as well in the thermometer, which he used on the 25th, as in two of the 26th, the bulbs were cracked in the experiment: they cohered however; nor was the least part of the bulb separated, but the congealed mercury seemed to adhere to all parts of the bulb. In the following experiments, he invariably found, that the mercury sunk lower, if the whole of it was congealed, than if any part of it remained fluid. It then generally descended to 680 and 700, but the bulbs were never without cracks; moreover, it descended to 800, and beyond even to 1500; but in this last experiment, the bulb was quite broke, so that the globe of mercury, thoroughly frozen, fell out, and by its fall, of about 3 feet, the globe of mercury became a little compressed; but in the former, only some parts of the bulb fell off.

Mr. Braun always found, that, *cæteris paribus*, the more intense the natural cold was, the more easy and more expeditiously these experiments did succeed.

In continuing these experiments he observed, that double aquafortis was more effectual than simple spirit of nitre; but that if both the aquafortis and Glauber's spirit of nitre, which he sometimes also used, were well prepared, the difference was not very considerable. When his aquafortis was frozen, which often happened, he found the same effects from the frozen parts, when thawed, as from that part of it, which remained fluid in the middle of the bottle. Simple spirit of nitre, though it seldom brought the mercury lower than 300 degrees, by the following method he even froze mercury with it.

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He filled six glasses with snow, as usual, and put the thermometer in one of them, pouring thereupon the spirit of nitre. When the mercury would fall no lower in this, he, in the same manner, put it in a second, then in a third, and so in a fourth; in which fourth immersion, the mercury was congealed.

Another very considerable difference presented itself in pursuing these inquiries, with regard to the mode of descent of the mercury. He constantly and invariably observed, that the mercury descended at first gently, but afterwards very rapidly. But the point, at which this impetus begins, is not easy to ascertain; as in different experiments it begins very differently, and sometimes at about 300, at other times about 350, and even further. In the experiment before-mentioned, in which the mercury fell to 800, it proceeded very regularly to 600; about which point it began to descend, with very great swiftness, and the bulb of the thermometer was broke. The mercury, however, was perfectly congealed.

He frequently observed another remarkable phenomenon; which was, that although the spirit of nitre, the snow, and the mercury in the thermometer, were previously reduced to the same temperature, upon pouring the spirit of nitre upon the snow, the mercury in the thermometer rose. But as this did not always happen, he carefully attended to every circumstance; from which it appeared, that this effect arose from his pouring the aquafortis immediately upon the bulb of the thermometer, not previously well immersed in the snow. He likewise observed another effect, twice only; and this was, that, after
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the thermometer had been taken out of the snow and aquafortis, the mercury continued to subside, in the open air, down as low as the congelation of mercury.

In the course of these inquiries, our professor found no difference, whether he made use of long or short thermometers; whether the tubes were made of the Bohemian, or the glass of Petersburg. Under the same circumstances, the same effects were always produced, making an allowance for the different contraction of the different glasses, under so severe a degree of cold. But if these tubes were filled with different mercury, there was then a sensible difference; inasmuch as mercury revived from sublimation did not subside so fast in the thermometer, as that did, which was less pure. He has even found, that he has been able to congeal the less pure mercury, at a time when he could not bring the revived mercury lower than 300 degrees: but this he would, till farther trials have been made, not have considered as a general axiom.

From these experiments, our author conceives it demonstrated, that heat alone is the cause of the fluidity of mercury, as it is that of water and other fluids. If, therefore, any part of the world does exist, in which so great a degree of cold prevails, as to make mercury solid, there is no doubt, but that mercury ought to appear there as a body equally firm and consistent, as the rest of the metals do here: that mercury, upon congealing, becomes its own ice, however different the mercurial ice may be from that of water, or other liquids. The idea of freezing does or can comprehend nothing more than

than a transition of bodies from a state of fluidity to that of firmness by the sole interposition of cold.

The ice of oily and saline bodies differs greatly from that of water, which is friable and easily broke, whereas that of mercury is ductile. And M. Braun proceeds to consider all bodies, which liquify by heat, as so many species of ice; so that every metal, wax, tallow, and glass, comes within his view, in this respect.

Mercury then is, in its natural state, a solid metal; but is fusible in a very small degree of heat. Every metal begins to flow in a certain degree of heat; but this degree is different in different metals. Pure tin begins to run at 420; lead, at 530; and bismuth, at 470, in Fahrenheit's thermometer: or, according to our author, lead liquifies at 320 above the cypher in his scale, which corresponds with 596 in Fahrenheit; lead at $170 = 416$ of Fahrenheit; bismuth at $235 = 494$; zinc requires a greater heat to melt it than will make mercury boil. Now, if it could be settled, at what point mercury would begin to be congealed, we should know the point at which it began to flow; as it has been long known, that water is either fluid or solid, as the heat of it is a very few degrees above or under 32 in Fahrenheit's thermometer. Just so metals become solid, at almost the same degree of heat in which they become fluid. But in mercury, the congealing point is at too great a latitude to be exactly determined; but our author estimates it to be about 469 degrees in his thermometer; at a less degree than which, he has not been able to observe the slightest congelation. Hence it follows, that the condensation or contraction, and consequently

consequently the diminution of the volume of mercury must be very great indeed. This is demonstrated by the great descent of the mercury in the thermometer, while it is freezing. But how great this diminution of the volume of the mercury is, cannot exactly be determined; and hence arises no small difficulty in determining its specific gravity, as this last must increase, as the bulk of the mercury lessens. Hence as mercury, even in its fluid state, comes of all bodies, platina excepted, the nearest to gold; in its solid state, it must still approach much nearer.

Our author had three thermometers filled with the most highly rectified spirit of wine. These not only corresponded exactly with one another, but, in less severe trials, corresponded reasonably well with those filled with mercury. But by the mixture of snow and spirit of nitre, which froze the mercury, he never was able to bring the spirit thermometers lower than 300. From hence it appears, that the heat, which will freeze mercury, will not freeze spirit of wine; and that therefore spirit thermometers are the most fit to determine the degree of coldness in frigorific mixtures, until we are in a situation to construct solid metallic thermometers with sufficient accuracy.

Our author made many experiments, to try the effects of different fluids, in his frigorific mixtures. He invariably found, that Glauber's spirit of nitre and double aquafortis were the most powerful. With oil of vitriol, the most ponderous of all acids, he was never able to congeal mercury. He likewise tried a great number of other fluids, both acid and spirituous, which though, when mixed with snow, produced cold, it was in very different degrees. He tried

tried a series of experiments to this purpose; but it was in weather far less cold than the preceding experiments were tried in, viz. between 159 and 153, by his thermometer. By these it appears, that spirit of salt pounded upon snow, increased the natural cold 30 degrees; spirit of sal ammoniac, 10; oil of vitriol, 35; Glauber's spirit of nitre, 58; aquafortis, 40; simple spirit of nitre, 30; spirit of vinegar, and lemon juice, made no remarkable difference; dulcified spirit of vitriol, 20; Hoffman's liquor anodynus, 32; spirit of hartshorn, 10; spirit of sulphur, 10; spirit of wine rectified, 20; camphorated spirit, 15; French brandy, 12; and even several kinds of wine, increased the natural cold to 6, 7, or 8 degrees. That inflammable spirits should produce cold, seems very extraordinary, as rectified spirit seems to be liquid fire itself; and what still appears more paradoxical is, that inflammable spirits poured into water, cause heat; upon snow, cold: and what is water, but melted snow?

Though not immediately relating to the principal purpose of this treatise, our author measured by his thermometer, when it stood in his study at 128 degrees, the heat occasioned by pouring different fluids into water. He found, that oil of vitriol produced 35 degrees; spirit of sea salt, 10; Hoffman's anodyne liquor rectified, 5; spirit of wine, 10. On the contrary, spirit of sal ammoniac mixed with snow, spirit of sulphur, and spirit of hartshorn, mixed likewise with snow, made no perceptible difference. Highly rectified chymical oils, mixed with water, produced no heat; nor with snow, no cold; as was tried in the oils of turpentine, amber, mint, and mother of thyme.

And here it is to be remarked, notwithstanding the contrary has been given out by some, that these chymical oils mixed with the most highly rectified spirit of wine, do produce no cold, either upon their mixture, or half an hour after.

It results from these experiments, that although there are many liquids, which can produce artificial cold, the nitrous acid is the most powerful; and mercury may be congealed by it, without any difficult process, at any time, when the heat of the atmosphere is not greater than 175 by the thermometer before-mentioned. And these experiments have not only succeeded with our author, but with many others; among whom, it may be sufficient to mention Messieurs Lomonosow, Zeiher, Aepinus, and Model, as these gentlemen have made themselves well known in the philosophical world. The nitrous acid was poured upon the snow, in no determinate quantity; sometimes a few drops were sufficient, sometimes it required a larger quantity. Snow seems to be more fit for those experiments, than pounded ice; as the former, from its loose texture, is of more apt and easy solution.

Hence it appears, that mercury is no longer to be ranked with the semi-metals, but as a perfect one, fusible, though with a much less degree of heat than any of the others. It agrees likewise with other metals; as their parts like it, when in fusion, attract one another, and run into globules, and, from a state of fluidity, pass into a solid state, not all at once, but successively, and vice versa. But it is worth inquiring, whether this metal, which agrees with all others, both in a solid and fluid state, has not the particular

property of boiling at a certain degree of heat, which is by no means to be observed in other metals. The degree of heat, in which mercury begins to boil, is not at 600 of Fahrenheit's scale, as is generally imagined; but at least at 709 of the same scale, which corresponds with 414 of our author's, whose cypher is at the heat of boiling water.

Both the boiling and freezing of mercury have this in common; that when it begins to boil, it rises with rapidity; and descends rapidly, when it begins to freeze. If, therefore, the mean term of the congelation of mercury is fixed at 650 below the cypher, and the term of its boiling at 414 above the cypher; its greatest contraction to its greatest dilatation, will be 1064 degrees of our author's thermometer, and 1237 of Fahrenheit's; as 212 is the point of boiling water in this last, and 32 the freezing one; which corresponds with 150, under the term of boiling water, in our author's. Hence every one will see the great alteration of specific gravity in frozen and boiling mercury, as, between one and the other, the tenth part of the volume is lessened.

It may be asked, why the mixture of snow and niturous acid does not run into a solid mass, and form itself into ice, but remain of a soft consistence, although actually much colder, than what is required to freeze aquafortis? We have already mentioned, that aquafortis freezes at 204 of our author's thermometer, which corresponds with 34 below the cypher of Fahrenheit's. The frigorific mass, in a degree of cold far below this, remained soft like a pultice. The cause of this extraordinary phenomenon seems to be no other than a continuation of the solution of

the snow, and its mixing with the nitrous acid. For as the production of cold depends solely upon the solution and mixture, it cannot happen, that this mass, which constitutes a fluid of a hard kind, should run into a solid consistence, so long as the solution and mixture continue.

And now, Gentlemen, it requires no small share of your indulgence, to pardon my having extended this account so far: but I have to plead in my excuse, that the subject of this work is intirely new, and replete with a vast variety of curious facts; all which exactly fall in with our excellent institution. For who, before Mr. Braun's discovery, would have ventured to affirm mercury to be a malleable metal? who, that so intense a degree of cold could be produced by any means? who, that the effects of pouring nitrous acid upon snow, should so far exceed those, which result from mixing it with ice; when snow and ice are produced from the same substance, and seem to differ only in their configuration? As Mr. Braun's work is in very few hands, I had reason to hope, that you would not be displeased to be informed, in a degree somewhat circumstantial, of these very extraordinary facts.

I am,

With the most profound respect,

Gentlemen,

Your most obedient,

humble servant,

April 18, 1761.

W. Watson.